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Commentary

Scope of Theory, Generalizability of Empirical Results, and Prospects for Research Strategy

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In his target article, Markman (2018) provides a valuable and thought-provoking critique of research on judgment and decision making, one that raises deep issues concerning the goals and methods of psychology. Markman addresses the strengths and weaknesses of laboratory research on decision making, naturalistic research, and possibilities for a conversation between the two that could advance the goals of both. Part of Markman's assessment comes early in the paper, when he writes that "there are few comprehensive models that help us to understand how decision makers will approach choice situations in natural settings" (Markman, 2018, p. 1). In the remarks that follow, I would like to enlarge on some of the points Markman makes concerning the two issues raised by that statement: the comprehensiveness or scope of theories in judgment and decision research, and the generalizability of laboratory studies to natural settings. Others have commented before on these matters. However, I am not aware of previous attempts to juxtapose these sets of issues in the way that Markman (2018) has done, much less to offer possible strategies for negotiating the dilemmas that are posed. After discussing the matters of scope and generalizability, I will add my own thoughts on research strategies.

Scope of Theory

Markman (2018, p. 2) points out that laboratory research in decision making has tended to focus on narrow aspects of mental processing, and even that such a focus is typically required. As an example, he cites early work to isolate individual heuristics of probabilistic judgment (e.g., the anchoring and adjustment heuristic) with subsequent work aiming to refine the individual heuristics by addressing still narrower processes (e.g., the origin of the anchors, the degree of adjustment). Indeed, Markman could have mentioned lines of research that have focused narrowly on specific phenomena rather than processes. Examples that come to mind include work on overconfidence, base-rate neglect, conjunction errors, and preference reversals, among many others. Sometimes lines of research become so focused that even specific stimuli, or minor variations of them, are re-used in multiple studies (e.g., the taxi-cab problem for base-rate neglect, the Linda problem for conjunction errors, a half-dozen specific pairs of gambles for preference reversals). The situation can give the appearance that the field is fragmented, even siloed, and one can argue that this reflects more than mere appearance.

Markman attributes this state of affairs to a number of causes, including the press of laboratory methods to isolate and control variables and the sociology of science that sets the incentives for scientists. He also gives some historical reasons. Specifically, he argues that the field shifted from an emphasis on computational level models of choice to algorithmic level approaches (in Marr's, 1982, use of these terms) in order to make finer-grained predictions about decision making, and in so doing the field came to focus narrowly on component processes of decision making (with some noted exceptions). (See Goldstein & Hogarth, 1997, for further details about the history of research on judgment and decision making, especially as psychology was turning away from behaviorism.) Markman (2018) also mentions "a (largely implicit) set of assumptions about how people make decisions" (Markman, 2018, p. 3), specifically that people first form a consideration set, then evaluate the options, and then select one. These assumptions form an implicit framework that has led researchers to focus on a particular task (viz., comparison of options).

I agree with this description of the implicit framework, but I would add that, at least until recently, the implicit framework has also included an adherence to a metaphor that "life is a gamble."

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That is, most important decisions in life involve options whose potential consequences vary in their desirability and cannot be predicted with certainty, and it is presumed that the options are evaluated according to the desirability and likelihood of those potential consequences. This metaphor encouraged researchers to use monetary gambles as stimuli, on the grounds that they capture the crucial elements of more interesting and important decisions in a pure, simplified, and manipulable form. Lopes (1983) likened decision researchers' use of monetary gambles to geneticists' use of fruit flies. (See Goldstein & Weber, 1995, for an extended discussion of the gambling metaphor in decision research.)

Markman (2018) mentioned researchers' use of gambles by way of illustrating the frequent use of content-impoverished stimuli in laboratory research, as opposed to the richer settings investigated in naturalistic research. However, I think it is worth emphasizing some of the implications for the issues under consideration here, namely scope of theory and generalizability of results. Ironically, most researchers who studied gambling behavior did so not because they were interested in gambling behavior per se, but because they thought the use of gambles as stimuli would *enhance* the generalizability of their results and the scope of the theories they were developing. After all, they were using stimuli that isolated and prominently displayed (what were believed to be) the quintessential elements of *any* important option: the likelihood and desirability of its potential consequences. Although concerns were voiced early about the comparability of playing gambles for real payoffs versus making hypothetical choices (e.g., Slovic, 1969), the pristine "clarity" of gambles was thought to broaden the applicability of results rather than limit them. Arguments about the content-specificity of decision processes came later (Goldstein & Weber, 1995).

I think there are a couple of implications to be drawn from this example. First, we should be cautious about our metatheoretic commitments to implicit frameworks. They can lead us astray and should be reconsidered from time to time, as Markman (2018) is doing in the target article when he questions the field's focus on comparison processes. Second, I think this example illustrates an additional source of fragmentation in decision research, beyond the use of divide-and-conquer research strategies and the incentives for researchers to focus narrowly. In this instance, if one accepts the evidence of content-specificity in decision processes, then the *data* force us to attempt to develop a taxonomy of content domains that elicit different processes and, at least for a while, study them separately.

More generally, an approach that begins by assuming simplicity or general applicability until the evidence shows otherwise, sometimes must face evidence that matters are more complex than previously assumed. Sometimes there are more pieces of the puzzle than one had imagined, and one strategy is to study them separately before (hopefully) putting things back together into a comprehensive theory. From this perspective, the multiplicity of narrow topics in judgment and decision research is not due solely to a "sociology of science [that] biases researchers against integrative frameworks" (Markman, 2018, p. 3), but to excessively parsimonious theories whose scope turns out to be more limited than hoped. As one more example, consider that early decision researchers sought to find a single overarching structure (along the lines of expected utility) that would account for virtually all decisions. However, sensitivity of decision behavior to variations in task environments (Payne, 1982) led researchers to accept that decision makers have a repertoire of decision strategies that they can apply in different circumstances. Studies of the contents of people's repertoires and the conditions under which one or another strategy would be applied may have contributed to apparent fragmentation of the field, but did eventually lead to integrative theory (Payne, Bettman, & Johnson, 1993).

In sum, although I would add to Markman's list of reasons for decision researchers to focus on a multiplicity of narrow topics, nevertheless I accept Markman's characterization of the field as all too lacking in attempts to construct comprehensive integrative theories. (The effort–accuracy framework of Payne et al., 1993, is one that Markman mentions as an exception.) The criticism is a longstanding one. A similar sentiment was voiced some 35 years ago by Wallsten when he wrote "one does not desire a separate theory for each heuristic or bias, but rather a single theoretical framework to predict the range of judgmental effects that are observed" (Wallsten, 1983, p. 23). Markman (2018) suggests that proposals for integrative frameworks are difficult to publish and may require book-length treatments. I suspect that the place to look for integrative proposals is in review chapters. A couple that come to mind are those of Einhorn and Hogarth (1981) and Weber and Johnson (2009). Einhorn and Hogarth's (1981) review decomposed the processes of judgment and choice into several subprocesses, specifically information acquisition, evaluation, action, and feedback/learning. (Similar proposals for phases of decision making and action have also been proposed by others. See, for example, papers on the Rubicon model of action phases, e.g., Gollwitzer, 1990.) Weber and Johnson (2009) structured their review around cognitive processes such as attention, encoding, memory, and so on. They argued that the field was moving away from the incremental adjustments to normative models that had "resulted in a proliferation of task-specific models," and toward an approach that promised to be "integrative by reducing a large number of models and insights to a manageable list of underlying perceptual, cognitive, and emotional considerations" (Weber & Johnson, 2009, p. 75). A single theory of the desired scope and power has not yet emerged, but I think it is fair to say that decision researchers have not abandoned the goal of obtaining such a theory, despite the preponderance of narrowly focused studies in the literature.

Generalizability of Empirical Results

Markman (2018) offers a nuanced discussion of the benefits and limitations of naturalistic research on decision making compared with laboratory research, which then leads into his treatment of internal and external validity and his suggestions for dealing with tradeoffs between them. A brief summary of this discussion cannot do justice to it, but let me highlight a few of Markman's points that seem particularly salient to me. Some of